Unemployment and Mortality:

A Longitudinal Study in the United States, 1968-1992

John N. Lavis, M.D., Ph.D.\textsuperscript{1,2,3,4}

1 Centre for Health Economics and Policy Analysis, McMaster University
2 Department of Clinical Epidemiology and Biostatistics, McMaster University
3 Institute for Work & Health
4 Population Health Program, Canadian Institute for Advanced Research

ACKNOWLEDGEMENTS

This paper was produced as part of my doctoral dissertation at Harvard University. My doctoral studies were funded in part by a National Health Ph.D. Fellowship from the National Health Research and Development Programme, Government of Canada (1994-7). I also received financial support through a Procter and Gamble Canada Fulbright Scholarship (1994-5), through a General Motors Doctoral Fellowship in Population Health from the Canadian Institute for Advanced Research (1996-97), and from the Institute for Work & Health (1996-98). The development of the mortality file used in this analysis was funded by a grant from the U.S. National Institutes of Health (5R01AG13036-02). I would like to thank the members of my thesis committee for their thoughtful advice during the design, execution and write-up of my dissertation. The committee included Joseph Newhouse, Paul Pierson, Alvin Tarlov and Paul Cleary, as well as Sol Levine before his death. I would also like to thank Jeremiah Hurley for his comments on an earlier version of this manuscript.

This paper has also been made available as Institute for Work & Health Working Paper 63. The Institute receives support from the Ontario Workplace Safety and Insurance Board. The views expressed in this paper are the views of the author and not necessarily the view of the Institute or the WSIB.
ABSTRACT

Background. Studies have consistently found evidence of an association between unemployment and all-cause mortality. The association appears to follow a gradient, with more or longer unemployment spells associated with higher mortality rates than fewer or shorter (or no) unemployment spells. The association persists after simultaneously adjusting for potential confounders measured at baseline. Studies have found that the local unemployment rate modifies the relationship, with two of four studies finding that the mortality rate for the unemployed group was higher in areas or periods with low unemployment rates.

Methods. I examined the relationship between unemployment and mortality in 2868 male household heads followed for up to 25 years and 2676 male household heads followed for up to 16 years as part of the Panel Study of Income Dynamics. I used annual measures of unemployment as time-varying variables in Cox regression analyses and controlled for annual measures of potential confounders. I looked for an overall association, for a gradient in the association, and for variation in the association according to the local unemployment rate and the time period.

Results. Men who were unemployed one or more times on the days of the annual survey had a higher hazard of death while in the labour force than men who were working on the days of the survey (hazard ratio 3.23 [1.61-6.48]) and had a higher hazard of death while in the labour force or retirement than men who were working or retired (hazard ratio 3.47 [1.41-8.56]). Men who experienced longer unemployment spells died earlier while in the labour force or retirement than those who experienced shorter (or no) unemployment spells (hazard ratio 1.03 [1.00-1.05] for a one week change in the duration of unemployment). No clear relationship emerged between the number of unemployment spells and mortality. Men who were unemployed on the day of the survey and lived in an area with a low local unemployment rate tended to die earlier while in the labour force than men who were working on the day of the survey or who lived in an area with a high unemployment rate or both (hazard ratio 3.50 [0.78-15.67]).

Discussion. Unemployment spells and their duration increase the hazard of dying. These associations persisted after adjusting for annual measures of potential confounders. The study provides suggestive support for targeting assistance at unemployed men in areas with low unemployment rates. Future research should explore the behavioural and biological mechanisms through which unemployment could affect mortality and examine possible ameliorating factors.

Key words: Unemployment, Mortality, Longitudinal Studies, Proportional Hazards Models, Socioeconomic Status
INTRODUCTION

A number of longitudinal studies have examined the relationship between unemployment and mortality.\textsuperscript{1-10} With one exception,\textsuperscript{9} these studies used government census or administrative data and death registries. These studies have consistently found an association between unemployment and all-cause mortality but, in part because of variations in the length of the observation period\textsuperscript{1} and in the level of aggregation (into cause-of-death categories) by which results are reported, they have not always found a statistically significant association between unemployment and cause-specific mortality. Studies have found an association between unemployment and mortality from cancer\textsuperscript{1,4,5,6,9} (especially lung cancer\textsuperscript{1,6}), circulatory diseases\textsuperscript{4,5,6,9} (though not consistently\textsuperscript{1}), and accidents, poisonings, and violence\textsuperscript{1,4,5,6} (including suicide\textsuperscript{1,4,6}). Four factors have hindered efforts to draw policy-relevant conclusions from this body of research: how these studies defined "exposure" to unemployment, whether and how they adjusted for confounding, whether and how they examined if this association varied according to the context in which unemployment was experienced, and whether and how they examined if this association could be explained primarily by the selection of unhealthy workers into unemployment.

Most studies have used definitions of "exposure" to unemployment that left room for substantial variation in "exposure" within the unemployed group, making it difficult to determine whether it is any unemployment experience, the number of such experiences, or their duration that warrants attention. Early studies defined "exposure" as being unemployed and seeking work on the day of the census,\textsuperscript{4} in the week before the census,\textsuperscript{1-3,5,7} or in the year before the census.\textsuperscript{6} More recent studies adopted more precise criteria: the individual had experienced a documented long-term unemployment spell (more than 300 days for those aged 16-54 and 450 days for those aged 55-64) anytime in a two and a half year period,\textsuperscript{8} the individual reported an unemployment spell that was not due to illness in a five year period after a period (again five years long) in which the individual was continuously employed,\textsuperscript{9} or the individual experienced an unemployment spell at least three months long in the three year period before a recession or at least one month long in any of the one year periods after the recession had begun.\textsuperscript{10} Only three studies examined the relationship between the number or duration of unemployment spells and mortality: one using employment status in the week before two consecutive censuses, one using the duration of an unemployment spell in the year before the census,\textsuperscript{6} and the other using data on whether an individual reported an unemployment spell of specified minimum duration in each of three consecutive time periods.\textsuperscript{10} All three studies found a gradient, with more or longer spells associated with higher mortality rates than fewer or shorter (or no) spells.
Few studies have adjusted for more than two potential confounders and none have adjusted for potential confounders at regular intervals during the observation period, leaving open the possibility that attention should be redirected from unemployment to another factor which is correlated with unemployment but more directly associated with mortality. Potential confounders include age, race, marital status, income, education, employment grade, social supports, health-related behaviours like smoking, and health status. Differences in the distribution of these confounders across the unemployed and employed groups could account for the observed difference in mortality rates between groups. Studies with simultaneous adjustment for more than two confounders measured at baseline included age, marital status, and one or more measures of socioeconomic status (employment grade, income, and education). Two studies also adjusted for baseline health status (though only one with reasonable measures) and one of these two studies adjusted for health-related behaviours. The association between unemployment and mortality persisted after adjusting for the baseline distribution of these confounders.

While the context in which unemployment was experienced may modify the relationship between unemployment and mortality, thereby suggesting how efforts could be targeted or modified according to the context, studies have either not used context-related measures or used the local unemployment rate as the only measure. In areas with low unemployment rates and in the more distant past (e.g. the 1950s versus today), unemployment may have been more unexpected for the individual and less pervasive in his or her social network, and so may have been associated with more negative health consequences. Also, in jurisdictions that provided meagre unemployment-insurance benefits, unemployment may have been more disruptive and therefore associated with more negative health consequences. As these examples suggest, an examination of the interaction between an individual's employment status and contextual factors may provide policy-relevant information, such as the means to identify and so provide assistance to those at most risk of negative health consequences (e.g. those experiencing unemployment in an area with low unemployment rates or at a particular time) or a target for more direct intervention (e.g. the generosity of a jurisdiction's unemployment-insurance benefits). Four studies have examined the interaction between an individual's employment status and the local unemployment rate: two studies found that the mortality rate for the unemployed group was higher in areas or periods with low unemployment rates; one study found that the local unemployment rate was not a statistically significant independent predictor of mortality; and another study found that the mortality rate for the unemployed group was higher in areas with higher unemployment rate. The finding from a comparison of different time periods could be explained by the selection of unhealthy workers out of the labour force (not just into unemployment) during periods with high unemployment rates.
No studies have examined directly whether the association between unemployment and mortality could be explained primarily by the selection of unhealthy workers into unemployment. That is, no studies have been able to differentiate precisely between workers whose job loss was health-related from those whose job loss was not. Unexpected firm bankruptcies and plant closings offer an opportunity to study workers whose job loss was not health-related,\textsuperscript{19,20} however, such events are relatively rare so the pool of affected workers in a longitudinal study (and the number of deaths in this group) is typically quite small. Alternative direct approaches that have not been attempted use either an alternate measure of unemployment which is not health-related or a combined measure of unemployment and another variable that acts as a proxy for a low probability that unemployment is health-related. The first alternative approach uses predicted "likelihoods" of unemployment (which should be unrelated to an individual's health status) rather than actual employment status (which may be directly related to an individual's health status). The predicted likelihoods are generated with variables like age, race, education, and the local unemployment rate or the local firm bankruptcy or plant closing rate. For example, a young, black, high-school drop-out who lives in a neighbourhood with a 20 percent unemployment rate might be expected to have a higher probability of unemployment than a middle-aged, white, college graduate who lives in an area with a 2 percent unemployment rate. The second alternative approach uses an interaction between employment status and a variable like union membership or firm size which may be protective against health-related job dismissal. Union members may be more likely held to a first-in / last-out rule and firm size has been shown to reduce the hazard of health-related job loss.\textsuperscript{21}

Instead of these more direct approaches, all studies have had to rely on one of two indirect approaches to examine whether the association between unemployment and mortality could be explained primarily by the selection of unhealthy workers into unemployment. The first indirect approach has involved looking more closely at the mortality experience of those "exposed" to unemployment. Some researchers have argued that if poor health were a major influence on the selection of workers into unemployment, the health (and relative mortality) of the unemployed group should improve over time as the proportion of sick individuals in this group declines (through death).\textsuperscript{1,4} Instead, studies found the same\textsuperscript{1,4} or more\textsuperscript{5} excess mortality in later years. But this argument ignores the possibility that the sick get progressively sicker. Researchers have also argued that the causes of death associated with unemployment do not suggest the types of diseases for which selection might plausibly work.\textsuperscript{1} For example, one study showed that the association between unemployment and all-cause mortality persisted after removing suicides and alcohol-related deaths.\textsuperscript{8} A third variant on this mortality-based approach has involved examining the relationship between a man's unemployment experience and the mortality of his wife
or other (often dependent) female household members on the grounds that the health of his wife and other household members could be affected by the stress of an unemployment experience but should be unrelated to the man's probability of losing his job (unless the man's care-giving activities interfered with his job performance). Studies found an association for wives but not a statistically significant association for other female household members. These studies did not address the possibility of assortative mating.

The second indirect approach has involved looking more specifically at the potential for the selection of unhealthy workers into unemployment. Some researchers have attempted to address selection concerns by adjusting for baseline health status. One study adjusted for the use of reimbursable medicines and the number of sick allowance days taken and another adjusted for self-reported health status and pre-existing diseases. Both studies found that the strength of the association was not appreciably reduced by adjustment, however, neither study adjusted for health status immediately prior to the unemployment experience. Other researchers have argued that if poor health were a major influence on the selection of workers into unemployment, the mortality rate for the unemployed group should be higher in periods or areas with low unemployment rates (when or where the unemployed group will likely include more unhealthy individuals). As mentioned previously, two of four studies found that the mortality rate for the unemployed group was higher in areas or periods with low unemployment rates. But aggregate measures like the local unemployment rate provide an inadequate test for selection at the level of an individual. Such measures do, however, capture important aspects of the context for unemployment experiences.

Using longitudinal follow-up data, I sought to address three of the four factors that have hindered efforts to draw policy-relevant conclusions from research on the relationship between unemployment and mortality. I examined whether men who experienced unemployment died earlier than those who did not experience unemployment, controlling for annual measures of potential confounders, and whether this association, if present, was stronger for those who experienced a higher "burden" of unemployment or for those who experienced unemployment in areas with low unemployment rates or during the first half of the study period. My hypotheses were as follows:

1) men who experienced one or more unemployment spells died earlier than those who experienced no unemployment spells, controlling for potential confounders;

2) men who experienced more unemployment spells died earlier than those who experienced fewer unemployment spells and men who experienced longer unemployment spells died earlier than those who experienced shorter unemployment spells, controlling for potential confounders; and
men who experienced one or more unemployment spells in areas with low unemployment rates died earlier than those who experienced one or more unemployment spells in areas with high unemployment rates and that men in an older birth-cohort (who would have experienced relatively more of their unemployment spells in the first part of the observation period) died earlier than men from a younger birth-cohort (who would have experienced relatively more of their unemployment spells in the second part of the observation period), controlling for potential confounders.

**METHODS**

I used data from the Panel Study of Income Dynamics (PSID) to test these hypotheses regarding the relationship between unemployment and mortality. The PSID is an ongoing annual survey of households begun in 1968, with data currently available through 1992. A number of analyses have provided evidence regarding the validity of the data and the absence of substantial non-response bias. I conducted the hypothesis-testing using two cohorts drawn from the full PSID sample: one cohort comprised 2868 male household heads followed for up to 25 years (1968 cohort) and the other cohort comprised 2676 male household heads followed for up to 16 years (1977 cohort). To perform the analyses I constructed a person-year file so that each man in a cohort had one line of data for each year he was in the study. The data included several annual measures of unemployment, time to event (death or censoring), annual measures of potential confounders (race, marital status, total family unit money income, family size, education, and employment grade), and two measures of the context for unemployment (the local unemployment rate each year and the time period). I conducted Cox regression analyses using time to event (with age as the time scale) as the dependent variable and the annual measures of unemployment as the principal independent variable, and I controlled for annual measures of potential confounders.

**Cohort Inclusion Criteria.** I restricted both cohorts to male household heads from the "core" PSID sample who were between the ages of 18 and 64 (inclusive) and both in the labour force and a survey respondent in the cohort base year (1968 or 1977). I focused on household heads because more detailed information is available for them and on men because most household heads were male. I used the "core" (instead of the "augmented") PSID sample because weights were available only for "core" sample members. I used the age-eligibility and labour force participation criteria to restrict the sample to adults who were at risk of losing their job and I used the survey respondent criterion to ensure that data were available on all men at the beginning of the observation period.
The two cohorts differed in the availability of specific measures and in the number of recorded deaths so they were used to test different hypotheses or to test the same hypotheses in different ways. Two differences were particularly salient: the unemployment measures and the comparison group. For the 1968 cohort I could determine whether a man was unemployed on the day of each annual survey but with the 1977 cohort I could also determine whether a man had experienced an unemployment spell at any time during the calendar year before each annual survey and the duration of the spell. Also, the 1968 cohort had enough men who died while in the labour force that the comparison group for those who were unemployed could be those who were working. The 1977 cohort, on the other hand, had a smaller number of men who died so that those who were working or retired were used as the comparison group. Thus for the 1968 cohort only deaths in the labour force were counted whereas for the 1977 cohort deaths in the labour force and in retirement were counted. Table 1 provides descriptive statistics for each cohort at cohort baseline.

Dependent variable. As the dependent variable I used time to event, where an event could be death from any cause or censoring. Deaths were coded as one of the reasons for non-response and information on the month and year of death has been gathered from surviving family members and from other contact persons named by the decedents prior to their death. The cause of death was not recorded. PSID staff began efforts to locate non-respondents in 1994 and through this effort they determined the month and year of death for some men whose vital status was previously unknown. One validation of the mortality data has shown that the mortality experience of the original PSID cohort closely approximated that of the United States as a whole, only slightly underestimating deaths in early adult life and in old age.

Censoring could occur either because a man failed to complete a survey (i.e. was lost to follow-up) or because a man completed a survey (i.e. was still alive) in the last year of the observation period (1992). To ensure the availability of reasonably complete data on all men in the study, a man was considered censored if he failed to complete one or more surveys and died at some point after the first missed survey period. This decision rule was applied to 72 men in the 1968 cohort and 27 men in the 1977 cohort. Men who failed to complete one or more surveys and then began completing surveys again were also considered censored. This decision rule was applied to 91 men in the 1968 cohort and 79 men in the 1977 cohort.

Time to event (in months) was determined using age as the time scale, not time since the baseline survey (i.e. not time-on-study). For the follow-up of a population for whom the start of the observation period is defined by completing a survey, not by a prognostically-meaningful
event like diagnosis or treatment initiation, using time-on-study as the time scale has been shown to result in biased hazard ratios. Thus the dependent variable is age at death or censoring (in months). Time to event was calculated using the man's age in the month and year in which the first survey was completed and the month and year of the event. For a man for whom I knew only a two year time frame in which death occurred and who completed a survey in the first of the two years, I used as the month of death the median month in the period after the last survey was completed and before the next survey would have been completed (operationalized as six months after the last survey was completed). This decision rule was applied to 12 men in the 1968 cohort and 2 men in the 1977 cohort. For those who were censored, I used as event times the month and year in which the man first failed to complete a survey, operationalized as the month and year in which the man last completed a survey plus the twelve months (or eight months for those censored in 1992) during which the man could have died before being labelled non-response for failure to complete the next year's survey or before I stopped counting deaths at the end of 1992. For a man for whom I knew only the year (but not the month) in which he last completed a survey (because the date of interview field was blank), I used the median month for survey completion in that year. This decision rule was applied to 7 men in the 1968 cohort and 10 men in the 1977 cohort. Table 2 provides the descriptive statistics for the survival data.

**Unemployment measures.** I used different annual measures of "exposure" to unemployment to examine the overall association between unemployment and mortality, any gradient in the association, and the variation in the association according to the local unemployment rate (see Appendix for an overview of these and other measures). For example, to examine the overall association in the 1968 cohort, I constructed a dichotomous variable that took the value 1 if a man was unemployed on the day of the survey, 0 if he was working that day, and "missing" if he was out of the labour force that day. For the 1977 cohort, the variable for a given year took the value 1 if the man had experienced an unemployment spell at any time during the calendar year before the given year, 0 if he had not experienced an unemployment spell at any time during that calendar year or was retired, and "missing" if he was out of the labour force for a reason other than retirement.

To examine any gradient in the association between unemployment and mortality, I constructed two mutually exclusive dichotomous variables for both cohorts and one continuous variable for the 1977 cohort only. The dichotomous variables were defined such that roughly equal numbers of men were allocated to the two "exposure" groups in each cohort. For the 1968 cohort, one variable for a given year took the value 1 if a man was unemployed for the first time (during the observation period) on the day of the survey and the other variable for that year took
the value 1 if a man was unemployed for the second or more times (during the observation period) on the days of the survey. Both variables took the value 0 if the man was working that day and "missing" if he was unemployed for a different number of times or was out of the labour force that day. For the 1977 cohort, one variable for a given year took the value 1 if a man experienced his first or second unemployment spell (of the observation period) during that calendar year and the other variable for that year took the value 1 if a man experienced his third or more unemployment spell (of the observation period) during that calendar year. Both variables took the value 0 if the man had not experienced an unemployment spell at any time during that calendar year or was retired and "missing" if he was unemployed for a different number of times or was out of the labour force for a reason other than retirement. For the 1977 cohort, I also constructed a continuous variable for the duration of any unemployment spell in a given year. This variable could take values between 0 and 52 weeks.

To examine the variation in the association between unemployment and mortality according to the local unemployment rate and the time period, I constructed one interaction term and one stratification variable for the 1968 cohort only. The dichotomous interaction term represents the product of the dichotomous employment status variable (which takes the value 1 if a man was unemployed on the day of the survey, 0 if he was working that day, and "missing" if he was out of the labour force that day) and a dichotomous unemployment rate variable (which takes the value 1 if the local unemployment rate was less than 6% -- that is, less than the median unemployment-rate category boundary over the observation period -- and 0 if it was greater than or equal to 6%). The interaction term takes the value 1 if a man was unemployed on the day of the survey and lived in an area with a low local unemployment rate, and 0 otherwise. The dichotomous stratification variable, constructed to identify time-period effects, represents each man's birth cohort. It takes the value 1 if a man was born between 1928 and 1950 (i.e. aged 18 to 40 at cohort baseline) and takes the value 2 if he was born between 1904 and 1927 (i.e. aged 41 to 64 at cohort baseline). Compared to the older cohort, the younger cohort includes more men who experienced unemployment spells in the second part of the observation period, a time-period when unemployment may have been less unexpected and more pervasive in his social network.

Covariates. I used one time-invariant (i.e. fixed) covariate and five time-varying covariates to control for confounding. The time-invariant covariate was race (black versus other). Time-varying covariates included marital status (married or common-law versus other), total family unit money income (in constant 1992 U.S. dollars24 or as a series of dummy variables representing less than $15,000, between $15,000 and $19,999, between $20,000 and $29,999, between $30,000 and $49,999, and between $50,000 and $69,999, with more than $70,000 the reference cat-
category), family size (as a deflator for total family unit money income), education (in years or as a series of dummy variables representing 0 to 8 years of completed education, 9 to 11 years, 12 years or high school, and 13 to 15 years, with 16 or more years the reference category), and employment grade (manual versus other). Measures of health-insurance coverage and health status were not available for either cohort. Total family unit money income includes income from all sources, such as work, social security or welfare transfers, and assets (e.g. rent, dividends, and interest). This variable was always used in conjunction with family size. The employment grade variable was constructed by combining single-digit occupation codes for manual workers together (craftsmen, operatives, labourers, and farmers) and those for non-manual workers together (professionals, managers, self-employed businessmen, clerical workers, sales workers, and service workers). Because education and employment grade rarely change from year to year, measures of these variables were often not repeated each year. For men with one or more years of missing data on their education or employment grade, I filled in missing values with that of the most recent year at which the variable was last observed. For the education variable, this decision rule was applied to 2341 (out of 2868) men in the 1968 cohort and 52 (out of 2676) men in the 1977 cohort. For the employment grade variable, this decision rule was applied to 1423 (out of 2868) men in the 1968 cohort and 1139 (out of 2676) men in the 1977 cohort.

**Statistical Analysis.** I estimated hazard ratios using Cox regression models with both fixed and time-varying covariates for the 1968 and 1977 cohorts. Annual unemployment measures were entered into the models as time-varying variables. Covariates were added and removed according to how they influenced the log likelihood. I retained in the model those confounders that reduced the log likelihood when added and increased it when removed. I stratified on birth cohort to identify time-period effects.

Descriptive statistics and Cox regressions were performed using individual weights that adjust for unequal selection probabilities and differential non-response. PSID’s original focus was the dynamics of poverty so the initial sample included a disproportionately large number of households in poverty during the late 1960s. The most recent year-specific weight was used for each man in a cohort and these weights were rescaled by dividing each man’s weight by the mean weight for that cohort. Because data are missing for some men in each cohort and most men have weights greater than 1, the weighted sample size for analyses (specifically those which drop men with any missing data) can be smaller than the unweighted sample size. Weighted estimates made from the sample are representative of the U.S. population (with the exception of recent immigrants).
Cox regressions were performed using a robust variance estimator that employs efficient score residuals to adjust for the repeated appearance of individual men in the risk pools. This variance estimator also effectively accounts for PSID's stratified design and cluster sampling (i.e. departures from simple random sampling) because anything unique to a cluster or stratum is adjusted for over time. Standard errors for descriptive statistics have not been adjusted to account for these design effects, however, so the true standard errors on these statistics are larger than those shown (Table 1).

RESULTS

When "unemployed on the day of the survey" (not unemployment spell in the calendar year before the survey) was used as the measure of "exposure" to unemployment, men who experienced one or more unemployment spells died earlier than those who experienced no unemployment spells, controlling for potential confounders. In the 1968 cohort (Table 3), men who were unemployed one or more times on the days of the survey had a higher hazard of death while in the labour force (hazard ratio 3.23 [1.61,6.48]) than men who were working on the day of the survey. In the 1977 cohort (Table 4), men who were unemployed one or more times on the days of the survey had a higher hazard of death while in the labour force or retirement (hazard ratio 3.47 [1.41-8.56]) than men who were working or retired (data not shown). Also in the 1977 cohort, men who experienced one or more unemployment spells during the year before each survey tended to have a higher hazard of death while in the labour force or retirement (hazard ratio 1.56 [0.82,2.98]) than men who were working or retired.

Men who experienced longer unemployment spells died earlier than those who experienced shorter (or no) unemployment spells, controlling for potential confounders, but no clear relationship emerged between the number of unemployment spells and mortality. In the 1977 cohort (Table 4), men who experienced longer unemployment spells had a higher hazard of death while in the labour force or retirement than men who experienced shorter or no spells (hazard ratio 1.03 [1.00,1.05] for a one week change in the duration of unemployment). In the 1968 cohort (Table 3), with unemployment measured on the day of the annual survey and only deaths in the labour force counted, a trend towards a reverse gradient was found in the relationship between the number of unemployment spells and mortality: men who experienced a second or more unemployment spell had a lower hazard of death while in the labour force relative to men who were working during the year (hazard ratio 2.20 [0.57,8.53]) than did men who experienced only a first spell (hazard ratio 3.50 [1.65,7.43]). In the 1977 cohort (Table 4), on the other hand, with
unemployment measured in the year before the survey and deaths in the labour force and in retirement counted, a trend towards a positive gradient was found: men who experienced a third or more unemployment spell had a higher hazard of death while in the labour force or retirement relative to men who were working or retired during the year (hazard ratio 1.83 [0.81,4.14]) than did men who experienced only a first or second spell (hazard ratio 1.25 [0.47,3.34]).

Men who experienced one or more unemployment spells in areas with low unemployment rates tended to die earlier than those who experienced one or more unemployment spells in areas with high unemployment rates but men in an older birth-cohort did not die earlier than men from a younger birth-cohort, controlling for potential confounders (Table 3). In the 1968 cohort, men who were unemployed on the day of the survey and lived in an area with a low local unemployment rate had a higher hazard of death while in the labour force than men who were working on the day of the survey or who lived in an area with a high local unemployment rate or both (hazard ratio 3.50 [0.78-15.67]). When stratified by birth cohort, men who were unemployed on the day of the survey had a hazard of death while in the labour force (hazard ratio 3.07 [1.54-6.10]) that was roughly similar to the hazard in the unstratified analysis (hazard ratio 3.23 [1.61-6.48]).

With one exception (income), the pattern of hazard ratios for possible confounders in multivariate Cox regressions followed the pattern in the univariate regressions. In the 1968 cohort, the (univariate) hazards of death by income category were 3.32 [2.20,5.01] for men with total family unit incomes less than $15,000, 2.07 [1.31,3.27] for those with incomes between $15,000 and $19,999, 1.53 [1.00,2.34] for those with incomes between $20,000 and $29,999, 1.15 [0.76,1.74] for those with incomes between $30,000 and $49,999, and 1.23 [0.77,1.96] for those with incomes between $50,000 and $69,999 (the reference category was incomes greater than $70,000). The (multivariate) hazards of death by income category were not significantly different from 1.
DISCUSSION

Using two representative samples of male household heads in the United States, this study confirmed that men who were unemployed one or more times on the days of an annual survey died earlier (while in the labour force or while in the labour force or retirement) than those who were not unemployed on the day of the survey and that men who experienced longer unemployment spells died earlier (while in the labour force or retirement) than those who experienced shorter (or no) unemployment spells. Moreover, the study did not detect any time-period effects and it found that men who were unemployed on the day of the survey and lived in an area with a low local unemployment rate tended to die earlier (while in the labour force) than men who were working on the day of the survey, men who lived in an area with a high unemployment rate or both. All of these associations persisted after controlling for annual measures of potential confounders. No clear relationship emerged between the number of unemployment spells and mortality.

Two main policy-relevant conclusions can be drawn from the study's findings. First, unemployment spells and their duration warrant attention because both factors increased the hazard of dying significantly. Men who were unemployed one or more times on the days of the survey had a hazard of death while in the labour force or retirement that was almost three and one-half times greater than men who were working or retired. Also, for every one week increase in the duration of unemployment, the hazard of death while in the labour force or retirement increased by 1.03. Second, these associations persisted after adjustment for annual measures of potential confounders so attention should not be redirected from unemployment to another factor which is correlated with unemployment but more directly associated with mortality.

Drawing conclusions about how the context for unemployment modified the relationship between unemployment and mortality was hindered by the relatively small sample size of the 1968 cohort in which this modifying role was examined. Testing for interaction effects and stratifying the analysis require larger sample sizes than unstratified testing for main effects. Although the hazard of death while in the labour force for men who were unemployed on the day of the survey and lived in an area with a low local unemployment rate was three and one-half times that of men who were working on the day of the survey or who lived in an area with a high local unemployment rate or both, the confidence interval associated with this point estimate was very wide (0.78-15.67). Thus the study provides only suggestive support for targeting assistance at unemployed men in areas with low unemployment rates (for whom unemployment may be more unexpected and less pervasive in their social network). The similarity in the hazard ratios
across the stratified and unstratified analyses may reflect a lack of power to detect a difference even if a difference existed. Thus the study also provides only suggestive support for not targeting assistance for unemployed men during particular time periods.

The study's findings include two anomalies: none of the analyses that involved using the more "complete" unemployment measure (unemployment spell in the last year rather than unemployed on the day of the survey) found a statistically significant result and no clear relationship emerged between the number of unemployment spells and mortality (as did in two previous studies\textsuperscript{5,10}). A measure, like unemployment spells in the last year, that captures all unemployment spells, not just those in progress on the day of a survey, more closely approximates the construct under examination - "exposure" to unemployment experiences. But when based on recall the measure can include a fair degree of measurement error,\textsuperscript{26} which may explain the lack of statistically significant results associated with this measure. The lack of clear relationship between the number of unemployment spells and mortality, on the other hand, may still reflect an underlying pattern. The first unemployment spell was more closely associated with deaths in the labour force than later spells but later unemployment spells were more closely associated with deaths in the labour force or retirement than first or second spells. Thus first spells may have affected causes of death with near-immediate consequences (like accidents, poisonings, and violence) and later spells may have affected causes of death with later consequences (like cancer or circulatory diseases). Unfortunately cause of death information is not available to confirm or refute this hypothesis.

Longitudinal follow-up (i.e. panel) studies, like PSID, provide an excellent testing ground for hypotheses about the relationship between unemployment and mortality, but panels designed for other purposes often require modifications to enhance their usefulness. The strength of such panels lies in their collection of data on unemployment and potential confounders at regular intervals during the observation period, which provides a more integrated accounting of health determinants through the life course. Several modifications to a panel study like PSID would make possible a more robust examination of the relationship between unemployment and mortality. First, cause-specific mortality data could help to determine the behavioural and biological plausibility of particular associations, such as the association between the number of unemployment spells and particular causes of death. Second, data on the voluntariness of unemployment could be used to develop a better measure of "exposure" to unemployment: involuntary, not voluntary, unemployment is presumably the only policy-relevant type of unemployment. Third, data on other potential confounders, like proxies for genetic background (e.g. age of parents' death), proxies for early childhood experiences (e.g. employment grade of parents), health-insur-
ance coverage, and health status (which, in a self-reported form, is available in PSID from 1984 onwards), could be used to rule out other factors that could be associated with unemployment but are more directly associated with mortality. Fourth, data on local firm-bankruptcy or plant-closing rates or on firm size could be used, respectively, to generate an alternate measure of unemployment that is not health-related (i.e., predicted likelihoods of unemployment) or a combined measure of unemployment and another variable that acts as a proxy for a low probability that unemployment is health-related (since large firm size appears to be protective against health-related job loss).

One direction for future research on the relationship between unemployment and mortality involves exploring the behavioural and biological mechanisms through which unemployment could affect health. I have identified six plausible mechanisms, of which two are behavioural, two are in conjunction with other health determinants (and may be behavioural or biological), and two are biological.

The two posited behavioural mechanisms -- increased risky health behaviours and decreased use of health-care services -- have not been consistently supported by longitudinal studies. For example, unemployed and early-retired men did not increase their smoking or drinking on becoming non-employed, although they were significantly more likely to gain over 10 percent in weight than men who remained continuously employed. Also, unemployed individuals increased (not decreased) their use of physician services and hospitals. However, these studies of health-care use were not conducted in the United States, where a loss of health insurance often accompanies unemployment.

The other posited mechanisms have not been well evaluated. First, job loss may be accompanied by a loss of social support through the workplace, and limited social support has been found to be associated with mortality. Second, job loss may be accompanied by a loss of income, and low income has also been found to be associated with mortality. Neither of these indirect mechanisms has been studied in an integrated way. Third, job loss may lead to stress-induced physiological changes, like increased cholesterol, which may have long-run health consequences. Fourth, job loss may lead to changes in the nervous, endocrine, and immunologic systems, which may also have long-run health consequences. Both of these biological mechanisms, especially the mechanism involving psychoneuroimmunology, have yet to be fully explored.
A second direction for future research involves the study of possible ameliorating factors. This research could take the form of longitudinal follow-up studies that make use of natural variation in the actions of governments or employers or the form of experimental studies that make available specific interventions to randomly chosen individuals or employers. For example, researchers could examine whether the generosity of unemployment-insurance benefits offered by governments or the efforts of employers to maintain social supports for laid-off employees reduce the negative health consequences of unemployment.

While a connection between unemployment and health mortality suggests the need for action, action could come from government or employers and could build on policy-relevant research. Governments can influence some potential ameliorating like, for example, the generosity of unemployment insurance benefits (if the association between unemployment and mortality is mediated in part through income), unionization rates (if unionization confers job security or other health-related benefits), and labour force participation rates (if the employment of a spouse makes a difference). Employers can also influence some potential ameliorating factors by, for example, helping to maintain social supports for laid-off employees (if the association between unemployment and mortality is mediated in part through disruption to social supports). Further research should seek to inform such considerations by government and employers.
REFERENCES


Table 1
Descriptive statistics for each cohort at cohort baseline [standard errors]

1 Adjustments have been made to account for probability of selection and differential non-response (using weights) but not to account for cluster sampling, so the true standard errors are larger than those shown here.
2 Family unit money income was converted to constant 1992 U.S. dollars using the urban/experimental U.S. Consumer Price Index (Bureau of Labour Statistics, 1997).
3 Income data were sometimes missing for 65 men in the 1968 cohort and for 36 men in the 1977 cohort.
4 Family size data were sometimes missing for 65 men in the 1968 cohort and for 36 men in the 1977 cohort.
5 Education data were always missing for 35 men and sometimes missing for 2388 men in the 1968 cohort and were sometimes missing for 62 men in the 1977 cohort. Most recent available education data were used to fill in gaps for those men with data that were sometimes missing, leaving 47 men in the 1968 cohort and 10 men in the 1977 cohort with education data that were sometimes missing.
6 Employment grade data were always missing for 27 men in the 1968 cohort and for 23 men in the 1977 cohort and were sometimes missing for 1498 men in the 1968 cohort and for 1254 men in the 1977 cohort. Most recent available employment grade data were used to fill in gaps for those men with data that were sometimes missing, leaving 75 men in the 1968 cohort and 115 men in the 1977 cohort with employment grade data that were sometimes missing.
Table 2
Descriptive statistics for the survival data

Adaptions have been made to account for probability of selection and differential non-response (using weights).
Adjustments have been made to account for probability of selection and differential non-response (using weights) and for the repeated appearance of men in the risk pools and cluster sampling (using a robust variance estimator that employs efficient score residuals).

Variables were added sequentially, starting with a baseline model containing only "unemployment spells" and with each new model containing one more variable. With the exception of the variable "black" (black versus other), the log likelihood for each new model was smaller (less negative) than the log likelihood of the previous model, and the log likelihood was larger (more negative) if any variable except "black" was dropped from the full model. Hence the most parsimonious predictive model corresponds to the full model without the variable "black" and includes the variables "unemployment spells" (see note 3), "married" (married or common-law versus other), "income" (total family unit money income in constant 1992 U.S. dollars), "family size" (number of men in the family unit), and "manual" (manual employment grade versus non-manual).

The "unemployment spells" variable can take several different forms, each of which uses employment status on day of survey and is time-varying (so that each man's employment status on day of survey in each year is used in the estimation). First, the variable "one or more spells" takes the value 1 if a man was unemployed on day of survey in a given year, 0 if he was working, and missing if he was out of the labour force. Second, the variable "first spell" takes the value 1 if a man was unemployed for the first time in the study period on day of survey, 0 if he was working, and missing if he was out of the labour force or experienced his second or more spell. Third, the variable "later spells" takes the value 1 if a man was unemployed for the second or more time in the study period on day of survey, 0 if he was working, and missing if he was out of the labour force or experienced his first spell.

The stratified model used the time-varying "one or more spells" variable and stratified on birth cohort (ages 18 to 40 at cohort baseline versus ages 41 to 64 at cohort baseline).

The interaction model used the time-varying "one or more spells" variable, "urlow" (local unemployment rate less than 6% versus local unemployment rate greater than or equal to 6%), and the multiplication of these two variables ("ue*urlow" takes the value 1 if a man was unemployed on the day of the survey and lived in an area with an unemployment rate less than 6% in the previous year and 0 if a man was working on the day of the survey or lived in an area with an unemployment rate greater than or equal to 6% in the previous year or both).
Adjustments have been made to account for probability of selection and differential non-response (using weights) and for the repeated appearance of individual men in the risk pools and cluster sampling (using a robust variance estimator that employs efficient score residuals).

Variables were added sequentially, starting with a baseline model containing only "unemployment spells" and with each new model adding one more variable. The log likelihood for each new model was smaller (less negative) than the log likelihood of the previous model, and the log likelihood was larger (more negative) if any variable was dropped from the full model. Hence the most parsimonious predictive model corresponds to the full model and includes the variables "unemployment spells", "black" (black versus other), "married" (married or common-law versus other), "income" (total family unit money income in constant 1992 U.S. dollars), "family size" (number of individuals in the family unit), and "manual" (manual employment grade versus non-manual).

The "unemployment spells" variable can take four different forms, three of which use whether a man experienced an unemployment spell in the previous year and one of which uses the duration of that spell. All four variables are time-varying so that each man's employment status in the previous year is used in the estimation. First, the variable "one or more spells" takes the value 1 if a man experienced an unemployment spell in the previous year, 0 if he was working, and missing if he was out of the labour force. Second, the variable "first spell" takes the value 1 if a man experienced his first or second spell in the study period, 0 if he was working, and missing if he was out of the labour force or experienced his third or more spell in the study period. Third, the variable "later spells" takes the value 1 if a man experienced his third or more spell in the study period, 0 if he was working, and missing if he was out of the labour force or experienced his first or second spell in the study period. Fourth, the variable "duration of spell" takes the value of the number of weeks in which a man was unemployed in the previous year, 0 if he was working and not unemployed in the previous year, and missing if he was out of the labour force.

First and second unemployment spells were grouped together so that the two "exposure" groups had roughly equal numbers of men.
Paper 98-5

Unemployment and Mortality: A Longitudinal Study in the United States 1968-1992

John Lavis